Editor-in-Chief

Dr. Cedric S. Raine
Professor of Pathology
(Neuropathology), Neurology & Neuroscience

Deputy Editor
Dr. Celia F. Brosnan
Professor of Pathology (Neuropathology)
Department of Pathology (F-140)
Albert Einstein College of
Medicine of Yeshiva University
1300 Morris Park Avenue
Bronx, NY 10461 U.S.A.
Fax: (718) 430-8710
Phone: (718) 430-2396
E-mail: jni@aecom.yu.edu

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Dr. R.W. Engelman
12901 Bruce B. Downs Blvd.
MDC 20
College of Medicine
University of South Florida
Tampa, FL 33612

Re: R-5333

Dear Dr. Engelman,

Your paper has been reviewed and has been found unacceptable for publication in the present form. However, if you are able to respond to the major revisions recommended by the reviewer(s), we shall be willing to reconsider it for publication. This reconsideration may involve another review. We enclose copies of the review(s). In the covering letter accompanying your revised manuscript, please indicate where changes have been made. **Please return a revised manuscript, one copy of the revised manuscript with the J.N.I. stamped title page as page 1 and a diskette.**

We also enclose an "author checklist" for you to complete and return to us with your revised manuscript.

Due to constraints imposed by our busy publication schedule, it is important that we receive your revised manuscript at your earliest convenience. Should your revised manuscript be received more than two months from this date, it will be processed for review as a new submission. Thank you.

Yours sincerely,

Dr. Cedric S. Raine
Editor-in-Chief
Journal of Neuroimmunology

CSR:jl
Encl.

REFEREE EVALUATION SHEET

Reviewer No. 1 **Editor's code and No. R-5333**

Reviewer's comments for authors:

"Hnt neurons express an immunosuppressive protein that blocks T-lymphocyte proliferation and interleukin-2 production" by Gower et al. is a thorough, well-written, interesting manuscript. I was impressed by the amount of characterization that was done on the immunosuppressive factor released by Hnt neurons. My main criticism of the manuscript is that, given the amount of characterization provided, the authors did not go a little further to finish the job. Specifically:

(1) It would have been helpful if the authors could have provided some sense of the "specific activity" of their immunosuppressive protein as they put it through the various columns. For instance, the authors could have focused on one biological assay (e.g., PHA-induced proliferation of PBMC) and, using 96-well plates they could have tested the supernatants at various dilutions, establishing an IC50. The specific activity could then be calculated as:

IC50 / protein concentration

This would have given the reader some sense as to whether the various procedures (e.g., blue-sepharose) were removing inactive materials without affecting the biological activity.

(2) The authors obtained a single active fraction from the isoelectric focusing and yet they did not run it on a PAGE gel (coomassie- or silver-stained) to determine the purity of the active fraction or to get a more defined molecular weight. I find it surprising that they were so thorough up to this point and left off the last step.